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ABSTRACT

Research can be related to reality, to meaning, and to decisions and decision makers. There are some assumptions about reality and research that are taken for granted: a) reality does indeed exist--which one must accept as true; b) research is an accurate portrait of reality--although no research has ever done justice to the original object; c) the methodology used has little effect on the outcome of the study--although, in truth, the methodology partly determines the answer; d) prediction is possible--although experience shows that things do change and that there are intervening accidents of history. The problem is an unawareness of the biases of assumptions. Similarly with regard to meaning and reality, the positive mania with statistical significance must be ended and the premise that statistical significance may have little relationship to practical significance must be acknowledged. Consequently, one tends to be a "closed shop" regarding his own expertise. In overall summary, alt ough reality is assumed to exist, the way of finding images of that reality is often inaccurate, narrow and, all too often, irrelevant and confusing. (JA)

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REALITY, MEANING, AND RESEARCH

by

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This paper discusses the relationship of research to reality, research to meaning, and research to decisions and decision makers. The two primary foci are on the assumptions we take for granted but which color our research and our findings, and on the difficulties decision makers have in using our findings.

Reality. The first assumption is interesting to deal with but difficult to resolve. It is that reality does indeed exist. Without getting into the problem presented by the Taoist teacher who fell asleep under a bow tree and dreamed he was a butterfly, and when he awoke he could not determine whether he was then a man dreaming that he was a butterfly, or now a butterfly dreaming he was a man, let us for the sake of mental and professional health, assume with most of Western society that indeed this building does exist and that the problems faced by decision makers do exist.

Second, after we assume that reality exists, we make another assumption which directly affects the reliability and validity of our research statements. We assume that our research is an accurate portrayal of reality. When we have said that we have described a phenomenon or that two variables are significantly different, what have we actually said? Have we truly described the phenomenon or have we given an image of the phenomenon? One is reminded of Plato's

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allegory of the cave where the actual object is never seen by the person, but only the shadow of the object as cast by a fire behind it. When we do research, we are in much the same situation in that what we describe is greater than our description. No research of which we are aware has ever done justice to the original object. We merely cast images on a wall. If this is true, then all our research statements must, by definition, be false to the extent that they are not completely true. This problem becomes even greater when we research abstract problems such as learning, power, value, growth, or our own field of juvenile delinquency. We should keep in mind that our descriptions of reality are merely approximations and that these approximations are heavily weighted by our methodologies.

Third, regarding methodology, our assumption is that the methodology used has little effect on the outcome of the study. It is the position of this paper that the methodology partly determines the answer. The empirical method as most commonly used leads to quantified and differentiated answers. If an engineer were to determine whether a dam should be built on a river, the answer would be in quantifiable terms such as terrainian conditions and water volume. If a poet were asked the same question, his answer would be in qualitative terms such as beauty, harmony, composition, etc. The realities presented would be quite different. In educational research our penchant has been to quantify rather than to qualify, and our realities reflect this bias. It has been the practice of educational research that if you can't count it, it isn't

worth counting and we rarely take into account the effect this has on outcomes.

A fourth assumption of educational research is that prediction is possible. We further assume that we can predict a particular variable not in terms of its greater context but rather as if the variable existed in a vacuum. We assume that all those artifacts which are not included in our study either have no effect on the studied variables or that they will not change or that any changes which occur will be random and slight and not affect the outcome. And yet, our own experience tells us that things do indeed change and that our predictions are at best inaccurate because of the intervening accidents of history. For example, predictions about everything from learning to juvenile delinquency were affected by the automobile, or the pill, or television which have no direct relationship to those predictions but which fall under the category of accidents or intervening variables which could not be accounted for in our predictions. What then are we saying when we attempt to predict? We are saying, "All things being equal, using my quantified approach and assuming that history will not intervene, I think the following things will happen again as they did before." The primary problem is that all things are not equal and, as Heraclitus said, "You can't put your foot in the same stream twice." This puts us in the difficult position of either dealing with such narrow constructs that history can't intervene, or making predictive statements that, at best, are misleading, or alternately, including broader tested methodologies in our research.

In summary, the problem is not that we make assumptions, but rather, that we often are not aware of themnor of the biases which they introduce into our research.

Meaning. The problem of the meaning of research is related to, but separate from, that of the reality of research. More and more the audience for which we perform research and evaluation is made up of non-university institutions. In the past, if we made assumptions which biased our data, few people really noticed since our audience was made up of our colleagues who functioned under the same assumptions and biases. However, if research is to become more than an interesting intellectual exercise and begin dealing with the audience of decision makers, then a number of changes have to be made.

First, we must end our positive mania with statistical significance. Not finding significance may be just as important to decision makers as finding it. Determining that we can not find relationships between variables is just as interesting as finding that there is a relationship. Our published findings are also becoming suspect. There is a decided penchant for editors to publish articles which have statistical significance above those which do not. There are some journals which state that if statistical significance is not reached they will not consider the article, and there are many researchers who will not submit articles which do not reach significance. If a journal rejects nineteen out of twenty articles and uses statistical significance as a major criterion, it is entirely

possible that the article which is published is the one out of twenty which is a Type I error. We are by no means suggesting that this is the case. What we are suggesting is that editors look carefully at the research implications of editorial policy since their policies may be misleading decision makers and researchers alike. Leaving aside the problem of trying to replicate only statistically significant studies, our present problem is that while decision makers are interested in making decisions, they are just as interested in not making incorrect decisions. Therefore, our mania with statisitcal significance is no more interesting to them than a finding of non-significance.

Second, finding statistical significance may have little relationship to finding practical significance. If the number of subjects in the study is sufficiently large, then almost any statistical difference or correlation can be statistically significant. It should always be remembered, however, that the decision maker deals with practicality and his decisions are based on such mundane criteria as the number of votes gained, the amount of money saved, or the efficiency increased. Statistical significance may or may not give him answers to those questions.

Third, because our audience has been made up of our colleagues, we have developed a series of statistical procedures which are highly refined. These procedures have three basic drawbacks for use in the field: 1. they are far too refined for the rather crude data found in the field; 2. many institutions are no longer willing to commission new studies to meet

the preconceived designs and methodologies of the researcher when most of the requisite data have already been gathered for administrative purposes and merely need to be analyzed; 3. the answers which are generated by refined statistical procedures are usually uninterpretable to the decision maker. A statistically significant "F test" or the factor loadings from a factor analysis are so much Greek to the decision maker. He is put in the position of either dismissing the data analysis as so much intellectual jargon, accepting the researcher's interpretation on faith, or bypassing the university researcher in favor of his own evaluation staff which is taught to discard everything they learned at the university during their first few weeks at the job.

Fourth, to our own detriment and that of the community we have maintained a closed shop regarding our expertise. This point is related to the third in that our procedures are so complicated both in their execution and their interpretation that it is difficult if not impossible for the decision maker to learn them. Further, however, we have made little effort to go into the field and teach our procedures and to learn procedures which may be of help to the decision makers and to ourselves.

Fifth, there is a feeling of "purity" among many researchers. The feeling is that if we go into the field and deal with decision problems, we can no longer consider ourselves pure or basic researchers. It is the opinion of these authors that universities will no longer be supported by a community which does not see some direct relationship between solving their problems and one of the most expensive institutions in their midst.

We should recall that our statistical methods have at their root the determination of methods to win at games of chance an "impure" and highly practical problem.

Finally, in overall summary, we choose to assume, with most of our colleagues, that reality does indeed exist. However, the way in which we go about finding images of that reality is often inaccurate and narrow and all too often is irrelevant and confusing. Our research has little relevance to decision making because it is too complex, it is largely uninterpretable, and our methods are inappropriate to the data or to the audience. If we wish to continue research as an interesting intellectual exercise for the amusement and edification of our colleagues, we can continue in much the same fashion. If, however, we wish to aid decision makers we must mend our ways and take as our base the problem as presented, not our preconceived notions and assumptions of classic research design.